# Minding the Small Change: Attention Constraints among Small Firms in Kenya*

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**Abstract**

Firms in developing countries tend to have low productivity. While capital market imperfections are likely part of the explanation, they are not the only one. In this study, we focus on one business decision among small firms in Kenya for which capital is not likely a binding factor: keeping enough change on hand to break larger bills. This is a surprisingly large problem among these firms, amounting to 5-10% of total profits. We conducted two experiments to shed light on why this happens: (1) regularly "reminding" firms about lost sales through weekly monitoring surveys, and (2) explicitly informing firms about the lost sales we observed. We find that both interventions lead to significant reductions in lost sales. The most likely explanation for these results is that firms face a limited attention constraint and that reminders help alleviate the problem.

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# 1 Introduction

Many firms in developing countries are less productive than their counterparts in richer countries (Hsieh and Klenow, 2009). Economists most often point towards missing markets, such as credit market imperfections or poor institutions, to explain this. Recent evidence, however, from the microfinance movement suggests that relieving capital constraints alone may not significantly improve firm productivity[1], despite estimates of high marginal returns to capital among small firms in developing countries (de Mel et al., 2008, 2009a; McKenzie and Woodruff, 2008; Kremer et al., 2011). While "traditional" constraints are likely part of the explanation[2], there is evidence of other constraints limiting firms' productivity, including limited human capital[3], present-bias (Duflo et al., 2011; Fafchamps et al., 2011), and informational constraints (Bloom et al., 2011).

As these types of constraints seem related to personal optimization and skills, they suggest a potentially strong role for business training. It is therefore somewhat surprising that the few studies which have evaluated business training tend to find limited effects. Most training interventions tend to find increases in business knowledge but no increases in profits.[4] On the other hand, the *rule of thumb* approach used by Drexler et al. (2010) which focused on simple rules rather than detailed business training, had somewhat larger effects on profits. Similarly, in Bloom et al. (2011), a management consulting firm assisted large Indian firms in implementing mostly simple management practice improvements.[5] In both cases, adopting the

[1]For example, Banerjee et al. (2010), Crepon et al. (2011), and Karlan and Zinman (2010) show limited effects of microcredit programs in India, Morocco, and the Philippines, respectively. The only study to find positive effects thus far is Attanasio et al. (2012) in Mongolia. While capital constraints may still be relevant, this suggests that other constraints are relevant as well.

[2]There is evidence, for example, that providing savings tools can lead to an increase in productive investment, i.e. see Dupas and Robinson (2011), Dupas and Robinson (2012).

[3]See for example Berge et al. (2011); Field et al. (2010); Karlan and Valdivia (2011); Klinger and Schuendeln (2011).

[4]For example, see Karlan and Valdivia (2011) among Peruvian microcredit clients, Bruhn and Zia (2011) among microfinance clients in Bosnia and Herzegovina, and Fairlie et al. (2012) among potential entrepreneurs in the United States. Drexler et al. (2010) also find limited impacts of a standard business training program among microfinance clients in the Dominican Republic.

[5]Bruhn et al. (2012) also find some evidence of an impact of consulting services on profits among small and medium-sized enterprises in Mexico. Firms received about 4 hours of services per week, and consultants spent

new business techniques were easy to implement and required minimal increase in attention on the part of the owners or managers.

A potential explanation for why relatively simple innovations may be more effective than more comprehensive and complex business strategies is that limited attention may constrain small firms' productivity, as modelled by Banerjee and Mullainathan (2010). Banerjee and Mullainathan discuss the role of finite attention on the productivity of the poor: if the poor face competing impulses to maintain attention on problems at home (they discuss, for example, the problem of a sick child), and if the poor face a tradeoff between investing their attention in work versus home production, then the comparatively greater challenges in maintaining their homefront may divert effort away from productivity, which can generate long-run poverty traps.

In this paper, we present what is to our knowledge the first direct test of whether limited attention reduces productivity in developing countries[6], by focusing on one particular business decision relevant for small firms in developing countries: how much change to keep on hand to break larger bills. In the area of study (Western Kenya), this decision leads to surprisingly frequent problems: in a survey of 508 small-scale firms, we estimate that the average firm loses 5% of profits on transactions which cannot be completed due to a lack of small change. Firms miss additional sales while spending over two hours per week searching for change. Using data on weekly profits and hours of work, we approximate that firms lose 5-8% of profits total from these two components.[7]). These lost transactions are all the more striking in that many

some of that time actually implementing changes, in addition to diagnosing problems and proposing solutions.

[6]There have been several studies of limited attention in regards to saving behavior. Karlan et al. (2011) show that reminding people of their savings goals increased the probability that people reached those goals in experiments in Bolivia, Peru and the Philippines. Similarly, Kast et al. (2011) show that reminders (through either regular peer group meetings or text message reminders) increase savings among microfinance clients in Chile. In contrast, Karlan and Zinman (2012) do not find any evidence that SMS reminders increase loan repayment in the Philippines.

[7]As explained in Table 1, we value the time spent fetching change at the average profit per hour as reported in our monitoring surveys (dividing weekly profits by 65 hours per week, the average hours reported in our background survey). This is likely a conservative estimate since we anticipate that changeouts occur most often during busy periods, when profits may be higher. An alternative back of the envelope calculation, using information on firms' reported lost sales while away fetching and average lost profits during changeouts, gives a

take place while the firms have cash on hand, but in too large of denominations. Therefore, unlike a variety of the other potential barriers to profitability, it seems unlikely that credit market failures, product supply chain failures, or limited demand markets could create this problem.[8] Similarly, the solution to the change problem - keeping some business income in a buffer stock of small denomination bills and coins - seems relatively intuitive and one for which formal training in business practices seems unlikely to be necessarily. Yet, despite the apparent presence of low-cost solutions to this problem, firms not otherwise involved in the study are willing to pay premia to solve the change problem: 40% of firms are willing to pay a 5% premium for change.[9]

One explanation for this result is that entrepreneurs run out of change and miss out on profits because competing problems divert their attention elsewhere, with potential consequences on the firms' productivity. We use two simple interventions to test whether attention constraints are a reason that firms run out of change so regularly. First, we visited firms once per week to administer a monitoring survey. In this survey, we asked firms about their profits, sales, and, most importantly, the number of times that they ran out of change (which we term "changeouts"), and the profits they lost from changeouts. We also ask how long they were away from the shop in the past week to get change from other businesses, so that we can estimate sales lost due to that time away. Finally, we asked about the amount of cash that the entrepreneur brought to work (in total and in coins). As the survey did nothing other than ask these questions of firms, at most they should have done nothing more than direct the

similar estimate of around 10% of profits.

[8]It may be the case that the distribution of bills and coins determined by the central bank is not optimal, and the cost of getting small change is high. For a history of the problem of small change scarcity, see Sargent and Velde (2002). This paper does not rule out that possibility, and anecdotal evidence even suggests that it is not always possible to get small change at local banks. However, it is not the case that the supply of change is such a limiting factor that businesses cannot improve their change management practices, as evidenced by the results.

[9]This suggests that other motivations for holding large bills such as fear of theft seem less likely.

entrepreneur's attention towards the change problem.

To evaluate the effects of the reminders, we randomized the date at which we started the changeout visits with firms. We can then estimate the effect of being visited more regularly by comparing lost sales at a given period of time between those firms that started the survey earlier to those that started later.[10]

Second, after following firms for two to three weeks, we calculated the amount of money each firm was losing due to insufficient change, and conducted an information intervention in which we went over the calculation with a randomly selected subsample of firms in the study. During this intervention, we informed firms of their own figures, as well as market averages.

We find that both interventions had large effects on the amount of change firms held. The number of changeouts declined by more than 30% due to the information intervention, and the reminder visits also reduced the number of lost sales due to insufficient change. This translates into less lost revenue and profits directly, and also indirectly from losing fewer sales while away from the shop fetching change during the day. These changes in the incidence of changeouts and associated sales and profits are accompanied by adjustments in change management behavior. Treatment firms report that they visit nearby shops for change less often, and that they give out change to nearby businesses less often.

Overall, our estimates indicate that behavioral responses result in an 12% increase in profits (significant at the 10% level). This figure is similar to the back-of-the-envelope calculation above on lost profits from changeouts directly and from time spent away from the shop fetching change. We further validate our estimates using objective measures of cash on hand and willingness to pay for change, both of which respond to treatment in predictable ways. Negative spillovers to control firms are not driving the main results, as demonstrated by an

[10]The design of this study was informed by the results in Kremer et al. (2011), which found non-experimental evidence that firms suffered fewer stockouts the longer they were interviewed about the costs of stocking out.

analysis using firms' GPS coordinates. Finally, at the conclusion of the project, we administered semi-structured endline surveys to respondents. Responses to those surveys reinforce our main results - respondents reported that both the changeout survey and the information intervention made the change problem more salient to them, and that they changed behavior in response (mostly by getting more change at the beginning or end of the day). Though the responses to such surveys should be taken with a measure of caution, respondents overwhelmingly report an increase in profits.

As the weekly surveys provided no skills training, nor any direct information, it seems most plausible that they served as a sort of reminder which helped firms aggregate the cost of lost sales, making the amount of money being lost to change more salient to firms. While the information intervention provided some new information (the firm's relative performance compared to market averages), the firm-specific information would have already been known to firms if they had processed the information. Our interventions, therefore, make a problem more salient to entrepreneurs while allowing these entrepreneurs to identity the best solution within their set of business practices. Thus, a likely explanation for our results is that firms were not paying attention to the lost sales to change, and the interventions reduced the cost of processing the information already available to them.[11]

Given the high level of complexity in solving inventory management problems, it is perhaps not surprising that attention to detail is imperfect. Moreover, though the study focused on just one business decision, the presence of a binding attention constraint may have additional implications for the ability of small firms to maximize profits. While the specific issue of change is surely not the largest constraint on the profitability of very small businesses in developing countries, it does provide a clean example of the role attention constraints have on profits in a

[11] Since each changeout is itself a reminder of the change management problem, our interventions facilitate business owners in aggregating and processing the information from each week - in the case of the weekly surveys - and over a longer time period in the case of the information intervention.

scenario where other constraints - such as capital - are less likely to be binding.

Two caveats are important in interpreting our results. First, as we focus on private gains to change management, the experiment cannot answer the question of whether lost sales from changeouts are simply captured by nearby firms or whether improved changed management would lead to more transactions overall. In fact, semi-structured interviews with the respondents suggest that most customers who experience a changeout simply purchase the item from another vendor (see Appendix Table A4), suggesting that many of the extra transactions that increase the profits of treatment firms may come at the expense of other nearby firms. This fact leaves unresolved the overall efficiency costs of changeouts at a market level as we do not know how often the availability of change results in additional transactions or how often it simply saves time on the part of the consumer. In our spillover analysis, we find no evidence that nearby control firms have lower profits or sales when they are near more treatment firms, but standard errors are also large enough that we consider this issue unresolved. A second important question we do not answer is whether the increased attention on change leads to declines in attention in other aspects on the entrepreneur's life. The evidence that the information intervention increased profits (at the very least, did not decrease profits) indicates that declines in attention in other aspects of the business, if any, did not come at significant cost. In the endline survey, only 1% of people reported paying less attention to something else. It is nevertheless difficult to rule out the possibility that less attention was paid to matters at home, which would still be consistent with our main result (that attention constraints are important for entrepreneurial profits) but may have important consequences for the welfare of the entrepreneur.

The paper is organized as follows. The experimental design, sampling, and the data are described in section 2. The results are presented in section 4, followed by evidence from

semi-structured interviews. We conclude in section 6.

# 2 Experimental Design and Data

## 2.1 Sample

The goal of this project was to assess the role of providing reminders to a representative set of small businesses in Western Kenya. To obtain a representative sample, we conduced several full censuses of markets to draw our sampling frame. More specifically, the project took place in two phases across 10 market centers. The first, larger phase took place in 7 market centers between October 2009 and June 2010, while the second phase place took place in 3 market centers between February and April 2011. To draw a representative sample of firms in each phase, we conducted a census of all businesses operating in the given market center.

In total, we identified 1,195 firms in the two censuses (884 in 2009-10 and 311 in 2011). As discussed below, a key aspect of our experimental design is to randomize the date at which we enrolled firms into the study. To do this, each firm identified in the census was given a randomly determined number, stratified by market center and a set of business types.[12] If a firm could not be enrolled (because they refused or could not be found), we replaced it with the firm with the next-highest random number.

Overall, we invited 793 firms (538 in 2009-10 and 255 in 2011) to participate in the project. We successfully enrolled 508 (64%) of these (309 in 2009-10 and 199 in 2011).[13] The firms in the study are fairly heterogeneous - our final sample consists of 24% fruit and vegetable

[12] Since there is a lot of heterogeneity in business type - there were 43 business codes in the census - we stratified by the 3 most popular (retail shop, fruit and vegetable vendor, and cereal trader) and a combined residual "other" category.

[13] While we only have detail on why firms didn't participate for part of our sample, a major reason is that there is tremendous turnover among these types of businesses. For example, in March 2010, we re-censused the firms we identified in October 2009 and were only able to trace 50% of the original businesses. This is consistent with Keats (2012), who finds turnover on the order of 40% over 6 month intervals in a nearby part of Kenya.

vendors, 37% other types of retail (i.e. shops, hardware shops, small vendors, etc.) and 34% services (i.e. small restaurants, repair, tailoring, barbers, etc.) - see Table 2.[14]

Also from Table 2, the typical business is small: over 60% of firms in the sample have only one worker, the owner. Even among the shops, which are generally larger businesses than the fruit and vegetable sellers, the owner is the only worker in 52% of cases. Moreover, only 16% of businesses have any salaried workers. In total, 56% of firms are operated by women.

### 2.2 Background on Costs of Not Holding Enough Change

As described in the introduction, lost sales because of insufficient change is a prevalent problem for these firms. Table 1 shows that over 50% of firms reported having lost at least one sale in the previous 7 days during our first interview with them. Firms also spent over 2 hours on average looking for change in the previous 7 days. Even firms that had not lost any sales in the past week spent over an hour and a half searching for change for customers during that time period. Panel B looks at estimates of total lost profits, including lost sales from changeouts and time spent fetching for change. This back of the envelope calculation suggests that the average firm loses around 5-8% of profits due to inadequate change, and this figure does not include other costs such as credit given out to customers because of insufficient change.

### 2.3 Experimental Design

To understand whether firms have changeouts in part because they are not aware of the profits they are losing, we conducted two main interventions. First, once firms were enrolled in the study, we visited them on a weekly basis to administer a short "changeout" questionnaire. This questionnaire asked firms a number of questions about change management, including the number of lost sales due to insufficient change in the previous 7 days, the value of these sales,

[14] The remaining 5% are classified as "other."

how much time they spent searching for change, and how often they gave or received change from nearby firms. The survey also contained measures of total sales and profits.

The experimental design is based on the idea that the survey itself may serve as a catalyst for firms to start altering behavior, as lost sales and profits due to poor change management become more salient (as was found, non-experimentally, in Kremer et al. (2011)). To measure this, we randomized the start date at which we began to administer the survey to firms. Thus, a comparison of firms which started earlier (and thus had more reminders through the surveys) to those firms which started later (and had fewer reminders) provides an estimate of the causal effect of the survey on behavior.

To provide variation in the number of times a firm had been visited, we randomly started surveying firms in waves. There were 12 waves in total (9 in 2009-10 and 3 in 2011). Typically, new waves were started about every three weeks, though there is some heterogeneity in the gaps between waves due to changing conditions in the field. There is also some heterogeneity in the length of the waves: the mean number of visits to a firm was 12, with a minimum of 8 and a maximum of 18.

The second intervention more explicitly emphasized the costs of insufficient change. After collecting data for about 6 weeks, we calculated the lost sales for each firm and then visited firms to give them information on their lost sales (the average number of lost sales and associated lost revenue and profit, the frequency and duration of leaving their shop unattended while searching for change, and the amount of goods given out on credit due to insufficient change). If firms are not aware of how each individual changeout is aggregating into total lost profits, this may also induce more attention towards change management. We also provided information on the average amount of lost profits for firms in their market area. Keep in mind, however, that our sample within each market area has a diverse set of businesses. It

is therefore likely that the individual's own information is much more informative than the market averages (which were not disaggregated by business type). Accordingly we view the intervention as primarily providing firms with information they already had but may not have processed.

The empirical strategy to estimate the effect of these two interventions on business practices can be summarized by the example below:

Figure 1

| | Week | | | | | | |
|---|---|---|---|---|---|---|---|
| | 1 | 2 | 3 | 4 | 5 | | 6 |
| Wave 1 (Information) | v1 | v2 | v3 | v4 | v5 | Information | v6 |
| Wave 1 (No Information) | v1 | v2 | v3 | v4 | v5 | Intervention (Wave 1) | v6 |
| Wave 2 (Information) | | | | v1 | v2 | | v3 |
| Wave 2 (No Information) | | | | v1 | v2 | | v3 |

Since businesses were randomly allocated to waves, we can estimate the impact of the reminders by comparing visits 4-6 in wave 1 to visits 1-3 in wave 2. The impact of the information intervention is straightforward: we would compare visit 6 for firms which received the information intervention compared to firms that did not among wave 1 firms. Empirically, we implement this in two ways: by comparing simple average across waves or between those who receive information and those who don't; and by running fixed effects regressions which utilize the experimental variation to estimate effects while controlling for secular time trends.

## 2.4 Data, Sampling and Balance Check

There are four main surveys used in the study: the "changeout" survey (which was discussed above), a background survey, a debriefing survey after the information intervention, and an endline. The background survey included demographic and background information, as well as risk and time preferences, access to credit, asset holdings, cognitive ability, and entrepreneurial

disposition.[15] The debriefing survey, administered to a subset of respondents, was designed to make sure that people understood the calculations. At the end of the survey, we asked firms a few questions about whether they were surprised by the results, whether they intended to change their behavior and, if so, how. The final component of the data is a semi-structured endline survey in which we asked respondents questions about their perceptions of the change problem and how they manage change.

Table 2 demonstrates that characteristics are similar for firms across the 12 waves of the intervention, and among firms given the information intervention versus those who were not. The specification in Table 2 is a regression of the variable described in each row on wave dummy variables, a dummy variable indicating whether the firm was sampled for the information intervention, an indicator for waves 7 through 9 to reflect the sample frame update, and the variables used in stratification: market fixed effects and dummy variables for the three largest business types. Column 1 shows the coefficient on the dummy variable indicating whether the firm received the information intervention. As the table shows, most of the firm characteristics are balanced. Firms that received the information intervention are less likely to have a bank account and less likely to keep financial records, both of which are marginally significant. Column 3 displays the $p$ value from a joint significance test of all the wave indicator variables. There are two characteristics which differ at the 5% level (risk aversion and whether the home has mud walls), and a third which differs at the 10% level (whether the firm employs salaried workers). These differences are likely due to random chance.

[15] The survey was administered during the study and not prior to the changeout survey. Therefore we use time invariant characteristics or characteristics which are very unlikely to be affected by the two interventions to confirm balance was achieved through the randomization in Table 2.

# 3 Econometric strategy

We estimate the effects of our intervention using three different specifications. The first two show specifications which pool multiple rounds together to focus on simple mean comparisons which address the impact of the changeout survey and the information intervention in a very transparent way. Our preferred specification uses the full nature of the randomization and the panel data (with firm fixed effects) to identify the effect of each visit and the information intervention.

## 3.1 Timing of waves

The changeout survey was administered to each wave starts at a different time. Therefore, at any given time, we have a group of veteran cohorts who we have been following for some time, and also a group of novice cohorts who we are interviewing for the first time. As a simple mean comparison, we divide each firm's observations into two groups: when it was in the "novice" cohort and its wave was the most recent to join the study, and when it was in a veteran cohort and there were other firms who were newer to the project. We then test whether mean outcomes are different for firms who are in their novice stage or firms in their veteran stage of tenure using the following specification:

$$\overline{y_{it}} = \beta_0 + \beta_1 veteran_{it} + \delta X_i + \epsilon_{it} \tag{1}$$

where $y_{it}$ is an outcome, such as the number of lost sales due to insufficient change, for firm $i$ with tenure $t \in \{novice, veteran\}$ and $X_i$ are controls for stratifying variables (market identifiers and business type controls). $\beta_1$ tells us the effect of the survey and therefore the impact of making the change problem more salient to firms. In constructing these means, we exclude all firm-

week observations where the firm has received the information intervention, so that we can be sure that the mean differences are not reflecting the fact that a veteran firm is also more likely to have received the information intervention. Standard errors are clustered at the firm level. As will be discussed in detail in section 4.5, not all firms were interviewed in all weeks, nor interviewed for the maximum number of visits in a given wave. A strength of this specification is that selective attrition is unlikely to be driving the results since outcomes are averaged over multiple weeks.

### 3.2 Information Intervention: Means

The information intervention, which provided firms with information on the average amount of money they lost over the previous weeks due to insufficient change, was administered once among randomly selected firms. All selected firms in a given wave received the information at approximately the same time. As a first look at the impact of the information intervention, we examine only the period after a particular wave has received the intervention, and ask whether firms who were randomly selected to receive that intervention are on average different from those who were not. Similar to McKenzie (2011), we control for the mean pre-intervention outcome to reduce noise in the specification. Specifically, we regress

$$\overline{y_i^{POST}} = \gamma_0 + \gamma_1 I_i + \gamma_2 \overline{y_i^{PRE}} + \delta X_i + \epsilon_{it} \tag{2}$$

where $I_i = 1$ if the firm was sampled for information, and $\overline{y_i^{POST}}$ and $\overline{y_i^{PRE}}$ are firm $i$'s mean value for the dependent variable, measured over the period following and preceding the information intervention, respectively.

### 3.3 Fixed Effects

The previous two specifications, while transparent, do not allow us to take advantage of the power inherent to the full design. Specifically, at any point in time, our randomization creates two random intensities of treatment: the number of visits that firms should have received, and whether or not that firm has received information. In principle, then, we can regress outcomes on how many visits a firm has received and whether it has received the information intervention, and both of these variables are exogenous, up to sufficiently flexible time trends. We can also include business fixed effects to limit the high degree of inter-business variability in the data. However, there is a small amount of non-random variation in actual visits - some of the firms we follow are quite mobile and were unable to be found every week (details discussed in section 4.5). Thus, we instrument the actual number of visits with the assigned number of visits, and estimate the instrumental variables equivalent of the following specification:

$$y_{it} = \gamma_1 N_{it} + \gamma_2 I_{it} + \mu_i + \lambda_t + \epsilon_{ivt} \tag{3}$$

where $N_{it}$ is the number of visits, and $I_{it}$ is whether we had given the firm information by time $t$. $\mu_i$ are firm fixed effects while $\lambda_t$ are month indicator variables. Standard errors are again clustered by firm. The first stage, predicting actual visits with assigned visits, is highly significant with a minimum $t$-statistic over 90. Identification of the number of visits is coming from exogenous variation across waves in the number of assigned visits while identification of the information effect (separately from time trends) stems from exogenous variation across waves as well as changes within waves.[16]

[16] An important point is that this specification for simplicity supposes a linear form in the effect of visits on outcome variables. Given that changes in outcomes within a wave before and after the information intervention also contribute to the estimation of that coefficient, it is difficult to interpret the coefficient on the information intervention as a "pure" effect of information versus the effect of information plus any non-linearity on the effect of repeated reminders. An alternative specification, omitted for brevity, which includes a non-parametric effect of repeat visits in order to isolate the information intervention effect by itself is available from the authors. The

# 4 Results

In sections 4.1-4.3, we discuss the impact of the two interventions on outcomes relating to changeouts, behavioral adjustments in change management, and profits / sales. The corresponding tables, Tables 3-6, show the three econometric specifications described above side-by-side for each outcome.

## 4.1 Changeouts

Columns (1) through (3) of Table 3 presents results from our three empirical specifications where the dependent variable is an indicator for having experienced a changeout in the past week. Column (1) reveals that firms who have experienced our survey for some time are, on average, 6 percentage points less likely to experience a changeout in the last week than firms who we have just enrolled in the project. Given the likelihood of experiencing a changeout among novice firms (the control) is 52%, this is an 12% change. Column (2) reveals that firms who were randomly selected for our information intervention are similarly 8 percentage points less likely to experience a changeout than those who were not selected, after the intervention has taken place. This represents a 20% change compared to the control group of firms that never received the intervention.

Finally, Column (3) demonstrates similar trends when business fixed effects are taken into account, utilizing the time series on each firm. The mean novice firm has been visited about 3 times, while the mean veteran firm has been visited about 7 times; our fixed effects estimates are thus broadly similar to the mean comparisons (though slightly larger in this case). We also show these results graphically in Appendix Figures A1 and A2 (for a host of outcomes).

results are quite similar. Also see Appendix Figures 1 and 2. Of course, even those estimates are hard to interpret as a "pure" information intervention effect because the intervention occurs within the context of firms who were already enrolled in the study, which is why we focus on the joint effects here.

These figures show coefficients and associated standard errors for regressions of changeouts on dummies for the visit number. The dose-response relationship between the number of visits and the frequency of changeouts is striking (as it also is for many of the other key outcomes).

Columns (4) through (6) of Table 3 look at the number of lost sales experienced in the past week. Once again, we see that veteran firms in our study and firms who have received the information intervention both experience fewer lost sales than those who are newly-enrolled or who have not received the information intervention. In particular, column (4) shows that veteran firms experience almost one fewer (0.8) changeout during this time, a 32% reduction, and firms that received the information intervention experience a 34% reduction in the number of changeouts. Moreover, this effect is similar whether we examine only mean differences or use the full fixed effects specification. Finally, treatment firms also lose fewer sales while away from the shop fetching change as a result of both interventions. These results are in columns (7)-(9), and the finding is robust across all three specifications.

Treatment firms also lose less income due to these lost sales, the direct effect of changeouts. Additionally, they also lose fewer sales while away from their shop to get change during the day, an important indirect effect. Appendix Table 1 shows that the information intervention reduced the value of lost sales (revenue) in columns (1)-(3) and lost profits from changeouts in columns (4)-(6). The impact of the information intervention is to reduce lost revenue by 42% and lost profits by around 33%. Both the veteran firm specification and the visit count coefficient in the full specification show that the reminder visits reduced lost revenue and profits.

### 4.2 Behavioral Adjustment

Firms can adjust their behavior in a number of ways in order to limit the frequency of changeouts. They can bring more cash into work in the morning; they can monitor their change flows

over the day; they can prioritize change for higher profit sales; or they can choose to participate or withdraw from change-sharing relationships among nearby entrepreneurs.

Columns (1) through (3) of Table 4 look at the log quantity of Kenya Shillings in cash brought into work in the morning, while columns (4) through (6) examine the log quantity of change brought in the morning. Both dependent variables demonstrate weak evidence that our intervention precipitated an increase in cash brought into work. In particular, on average, veteran firms bring in about 13% more cash and 25% more change than newer firms, though only the former coefficient is statistically significant. There is no evidence, however, that the information intervention affected the amount of change brought in, as the standard errors are large. Columns (7)-(9) of Appendix Table A1 shows that the number of changeouts which occur while firms have cash on hand - just not the right bill denominations - also declined, consistent with improved change management. All in all, we can conclude that there is some weak evidence that firms overall are changing their behavior on the margin of bringing cash into work.[17]

A second dimension of possible behavioral adjustment is in how often firms share change with other market members. Table 5 examines this possibility, where columns (1) through (3) examine how frequently the business received change from other businesses and columns (4) through (6) examine how many times the business gave change to other businesses over the past week. Veteran firms in our intervention receive change on average 2.4 fewer times per week, and also share change with other businesses on average 1.1 fewer time per week. Similarly, upon receiving the information intervention firms begin receiving change an additionally 1.6 fewer times per week. The fixed effect specification in column (6) more precisely estimates the impact of the information intervention on change given to other businesses, suggesting firms sharing

[17] We note that these effects are somewhat heterogeneous across market centers. In the 2009-10 markets, we observe statistically significant increases in cash brought into work as a result of our interventions, while in the 2011 markets we do not.

almost one fewer time per week. Therefore, we observe firms adjusting their sharing behavior in response to our intervention. Logically, as these firms are sharing change primarily with other nearby firms, we may anticipate the presence of spillovers in these variables in particular. In section 4.6, we both document that sharing behavior is one of few variables with precisely identified spillover effects, and discuss how these spillovers may bias these estimates away from zero and change interpretations.

### 4.3 Profits

We have two measures of profits which may be affected by our interventions. Firms were asked about quantities sold in the past two hours and also total profits in the past two hours. While these variables are likely measured with substantial error, we note that de Mel et al. (2009b) found a simple question on profits was about as accurate as a much more detailed accounting-based measure. We also anticipate the reports from the previous two hours to have less measurement error than the entire last week.

Columns (1) through (3) of Table 6 examine log sales over the past two hours, while columns (4) through (6) examine log profits. In both cases, the data are too noisy for the simple mean comparisons to deliver significant differences across treatment and control firms. However, in the fixed effects specification, which uses the full power inherent in the randomization, we identify a significant (at the 10% level)[18] increase in profits associated with the information intervention, and a positive but insignificant increase associated with additional visits. Firms who received our intervention experienced about a 14% increase in sales and 12% increase in profits over the past two hours. This magnitude is similar to our back-of-the-envelope calculation regarding the total volume of profits which are lost due to the changeout problem

[18] It is well established in this literature that profits of micro-enterprises are measured with a significant amount of noise, as discussed in de Mel et al. (2009b); Bruhn et al. (2012). Sensible trimming of the profit measure results in more precise estimates (significant at the 5% level) of similar magnitude.

including the direct losses from changeouts and indirect losses from time away from the store while fetching change. At a minimum it shows that responding to our interventions did not lead to a reduction in profits. It is also consistent with the hypothesis that our interventions lead the firms to partially solve the changeout problem, and suggests that motivations for changeouts which could lead to increased profits (for example, as a bargaining tool over prices) are not dominant in our context.[19]

### 4.4 Reporting Bias

A concern with the results described above is that the outcomes are all based on self reports. There is therefore the potential for reporting bias. There are two main possible ways in which systematic reporting bias could drive the results. First, firms may have wanted to shorten the length of the survey. For example, if a firm reported experiencing no changeouts over the last week, then a number of subsequent questions are skipped. As the firm learns the questionnaire better, the firm may be less likely to report a changeout to minimize time spent with the enumerator. Of course, the information intervention results are not subject to this concern. Second, there may be a desire on the part of firms to please the surveyors.

We provide multiple pieces of evidence that reporting bias is not driving the results reported above. First, it is unlikely that the results are driven by a desire to reduce the amount of time spent answering our questions. We see an impact of the changeout survey both on questions that lead to skipping of questions (whether the firm experienced at least 1 changeout in the last week) as well as questions that do not reduce the length of the survey (for example: how many changeouts they experienced; and how many times they gave change

[19]This is not surprising since in this context the typical changeout is a customer attempting to purchase a 20 Ksh item with a 200 Ksh bill. It is unlikely that having no change (or claiming not to) could raise the price that dramatically. Moreover, Appendix Table 4 also indicates that according to the endline survey, few customers end up buying more when the firm runs out of change.

to other businesses). Second, we note that if desirability bias is behind our results, it is quite sophisticated: based on our back-of-the-envelope calculations, entrepreneurs are getting relative magnitudes about right across the board.

In order to address the concern more directly and provide an objective measure of behavioral change, in the second data collection round conducted in 2011, we "audited" firms. After asking them how much cash and change they had on hand as done in the changeout survey, we paid firms a small amount to show us all the money they had on hand, by denomination. We use this as an objective measure of cash management and look at the impact of the two interventions on the amount of change on hand as measured during the audit, and examine data quality more generally. As we have only a small number of audits per firm, we focus on the specification

$$y_{it} = \beta_1 veteran_{it} + \beta_2 info_{it} + \delta_t + \varepsilon_{it}$$

where standard errors are clustered at the firm level. Table 7 shows that the changeout survey, by comparing waves which started earlier in a given interval to firms which started the survey later, led to both more cash on hand and more change on hand. Table 8 shows reassuring evidence that firms tend to report cash on hand truthfully: the objective measure of cash on hand (the dependent variable in columns (1) and (2)) is significantly correlated with reported cash on hand. This provides additional evidence that firms are providing fairly accurate information on cash on hand. The correlation between the audit measure of change on hand and the self-reported quantity of change on hand is, however, less significantly correlated. The information intervention is not correlated with this objective measure in this sample (standard errors are large in both cases); without developing this formally we note that the 2011 sample experienced weaker effects of the information intervention on many outcome variables. Given that the 2011 sample was drawn separately and differs from the 2010 sample on several di-

mensions, it is difficult to draw strong inferences about why information may have been less effective in this sample.

Finally, if our interventions were effective, they ought to have affected the extent to which respondents are willing to pay for change. We note that precisely how they affect the willingness to pay is ambiguous, and depends on whether firms were already aware of the problem of changeouts and to what extent the interventions led firms to solve that problem. For example, if our interventions alerted businesses to the idea that changeouts were a problem, then they ought to increase the willingness to pay for change (and willingness to pay for change should be low among firms who were not exposed to our interventions). In contrast, if firms were already aware that changeouts were a problem, and our interventions helped them in solving the problem, then our interventions ought to have lowered the willingness to pay for change from a high baseline. We may consider four possibilities:

| Control Firm Willingness to Pay | Treatment Firm Willingness to Pay: High | Treatment Firm Willingness to Pay: Low |
|---|---|---|
| High | Recognized Problem<br>No Evidence of Learning | Recognized Problem<br>Solved by Treatment Firms |
| Low | Treatment Firms<br>Learned about problem | No Evidence of Learning |

To test whether willingness to pay changed in response to our intervention, we implemented a short willingness to pay module on a subset of 56 firms, some who had been enrolled in the experiment and some who had not. Firms were offered change amounts of 90 shillings, 95 shillings, 98 shillings, 99 shillings, 100 shillings, 105 shillings, and 110 shillings, in turn, with the price fixed at a 100 shilling note. At the end, we randomly chose one offer and actually

implemented it. We note that 0 firms were willing to pay 100 shillings for 90 shillings in change, and every firm was willing to accept change offers at 100 shillings or more in change for a 100 shilling note.

However, there are large differences in treatment and control across the range of 95-99 shillings in exchange for a 100 shilling note. Among firms not enrolled in our study, table 9 shows that 40% were willing to pay a 5% premium, and 60% were willing to pay a 2% premium for change. Among firms who were enrolled in the study, those fractions fall sharply, so that only 15% of study firms were willing to pay a 5% premium and only 22% of study firms were willing to pay a 2% premium. Despite the small sample, those estimates are statistically different at the 1% level. This is consistent with the hypothesis that firms in general are aware that change is valuable, and that our intervention is affecting change management so that study firms are solving the problem on their own.

Taken together, these three pieces of evidence all suggest that the results presented in Tables 3 through 6 are not driven by systematic reporting bias but rather as changes in the frequency of changeouts as a result of the intervention.

### 4.5 Attrition and Threats to Validity

Given that this study involved finding firms at regular intervals to administer a survey, a natural concern would be that the results could be affected by attrition. We examine this in column (1) of Appendix Table 2. We find that 97% of firms had at least 1 visit after the information intervention, and that attrition is not differential between the information and control groups. In addition, though not reported in the table, 99% of firms had at least one visit while they were the "veteran" wave. Specifications (1) and (2) are therefore unlikely to be driven by attrition.

The remaining concern would be that the firms that stay longer are naturally those with

fewer changeouts. We view this as very unlikely, however, given the similarity of the estimates with specifications (1) and (2), and the graphical evidence presented in Appendix Figures 1 and 2 which show a clear dose-response relationship between the number of visits and the frequency of changeouts. Furthermore, we instrument for the number of visits with the ideal number of visits in all our fixed effects specifications. Note that there is not a tremendous amount of partial attrition: there are 410 missing observations for this exercise (out of a total of 5,238 visits which should have occurred), or about 7.83%. Moreover, given that we find similar results for the simple comparison of means in which we simply collapse averages, we feel confident that our results are not driven by attrition.[20]

### 4.6 Spillovers and Equilibrium Effects

We anticipate that our treatments may have had two types of spillovers, leading to equilibrium effects and potentially affecting the interpretation of our estimates. First, firms who are being treated may discuss our changeout survey with other nearby firms. To the extent that discussions which take place between entrepreneurs serve to remind untreated firms of the changeout problem, we may expect to see attention constraints slacken market-wide when our enumerators conduct interventions. Second, changes in treatment firm behavior may directly affect firms who are not being treated. Specifically, if firms are capturing a larger fraction of potential sales due to improved change management, then that may result in other nearby firms experiencing fewer sales. These two types of spillovers should have opposite effects on our estimates: learning spillovers would result in conservative estimates. In contrast, spillovers due to behavioral changes of treated firms would indicate that our treatments affected market equilibria, so that our estimated differences are averages of the improved outcomes for treatment

[20] A related concern would be non-compliance with the information treatment. We check this in column (2) of Appendix Table A2. Ninety-five percent of those sampled for the information intervention actually received it.

firms and the worsened outcomes for control firms.

In the 2011 wave of the survey, we collected GPS coordinates of study firms, allowing us to determine the effects of being near treatment firms on various outcomes. We focus on differences in reported changeouts and behavior during firms' first visit as a function of neighbors' treatment status since these visits serve as the cleanest control group.[21] Because when firms are enrolled into the study is random, on each firm's first visit there is a random fraction of the other nearby firms who have already been enrolled into our study. For different outcome measures $y_{i1}$, we therefore regress:

$$y_{i1} = \beta_1 NumCloseFirms_{i1} + \beta_2 ShareTreated_{i1} + \lambda_t + \varepsilon_{i1}$$

where $\lambda_t$ are month indicator variables as before, $NumCloseFirms_{i1}$ indicates the number of firms within 100 meters of firm $i$, and $ShareTreated_{i1}$ indicates what fraction of those firms are already enrolled in our study. Table 10 presents the results of this estimation.

There are some important spillovers from our treatment on other nearby firms. First, Columns (1) and (2) reveal that firms experience fewer changeouts when other nearby firms are treated, and the effect on the number of changeouts is significant at the 5% level. This would lend support to the learning channel for potential spillovers, and suggest that our estimates underestimate the effects of treatment on changeouts. There are no reported differences between firms in more or less dense treatment areas on the amount of cash or change that entrepreneurs are bringing into the shop, suggesting that this learning does not affect behavior on that dimension. However, columns (5) and (6) reveal how these firms are nonetheless able to reduce

[21] This restriction prevents us from being able to reliably separate spillovers from the information intervention to those from the changeout survey intervention. Only one wave of firms in 2011 were enrolled after any had received the information intervention, rendering the share of nearby firms who had received the information intervention almost completely collinear with the share of nearby firms who were treated, the number of nearby firms, and month dummies (the $R^2$ from a regression of share of information firms on those variables at the time of the first visit is 0.91).

changeouts: they are sharing change more frequently (by both giving and receiving change more times per day). This suggests that our estimates on sharing change can be interpreted as both a reduction in sharing behavior by treatment firms, and an increase in sharing behavior by non-treated firms. The results are consistent with the following interpretation: the learning effects appear to be insufficient to change practices outside of the work place (since firms do not adjust the amount of cash brought in), but result in a higher frequency of transactions at the work place. We note that if treatment firms are refusing to share change because of the salience of treatment, the overall pool of potential firms to get change from would decrease, which could contribute to the higher frequency change sharing of untreated firms. Finally, column (7) and (8) reveal that there are no spillover effects on profits that we can estimate.

Overall, we interpret our spillover analysis as evidence that learning does take place, and that equilibrium spillovers are important in this context, without affecting our primary conclusions on the presence of attention constraints.

## 5 Supporting Evidence from Semi-Structured Interviews

The results discussed above present relatively clean evidence that reminding people of the change problem and providing information about how much money they were losing changed behavior. However, to supplement our main results, we performed two "debriefing" surveys during the experiment. The first was administered to a subset of individuals after they had received the information intervention. The second was administered to a subset at the very conclusion of their participation, and included both those who received information and those who did not.

Panel A of Table 11 presents results from the debriefing interview after the information intervention. Generally, the results are supportive of our main findings. Interestingly, nobody

reports that they do not keep change because the returns are higher to some other investment (such as inventory). People were much more likely to say it was because it was hard to get change or that it takes a long time to get it. Also consistent with our results, people seemed to be "surprised" by the results of the study - 83% reported that the amount they lost due to changeouts was more than they had anticipated, and 84% said that holding more change had higher returns than the next-best alternative investment. Finally, 85% reported that they planned to change their behavior after the intervention, with most saying that they would be sure to get change either after they finished work in the evening or before they started work the next day. Only a minority reported that they would have to take money out of other investments.

Panel B of Table 11 paints a similar picture in regards to the monitoring visits themselves. Eighty-six percent of individuals report that the visits changed the way they think about change, and 75% report that they changed their behavior. As with the information intervention, the most common changes were to get more change in the evening or the morning, and to bring more change to work from home. Forty-two percent of individuals also say they are now less likely to give out change. Perhaps more importantly, the responses are supportive of our finding of an increase in profits - 62% report making more money ultimately from these behavioral changes, and only 3% of these individuals report that there was any adjustment period to higher profits. At the same time, relatively few people (23%) report that deciding how much change to bring is difficult, potentially suggesting that it does not require major changes in business practices to adjust change behavior. Finally, we asked people if they had to take focus away from something else to focus more on change - only 1% say that they did.

Finally, the endline survey gives some insight into other questions raised by this research. First, Appendix Table A3 shows that most individuals save their profits for their day at home

(at least for a few days), and that many people report difficulty saving. Only 46% of people separate their business and personal cash, and 87% report spending more on good days. These figures (which are very similar to those reported in Kremer et al. (2011)) show how difficult it is for people to save money when profits are entirely in cash and must be saved at home. It seems conceivable that people may find it hard to adequately set aside money for change for the next day.[22]

## 6 Conclusion

Small businesses in developing countries tend to have low productivity, and many businesses seem to be unable to take advantage of potentially profitable investment opportunities. A natural explanation for this is that firms are constrained by physical or human capital constraints. Yet recent evidence suggest that simply lowering these barriers by expanding credit access or by providing basic business training have, at best, limited impacts. Are there other factors which constrain firms' profitability?

In this paper, we argue that limited attention may be another important constraint on small firms in poor countries. To study limited attention, we focus on a business decision for which capital is not likely a constraint, and which firms are well aware of without any formal training - how much change to keep on hand to break larger bills. Though this is a relatively straightforward aspect of running a business, it may be an aspect that is overlooked if owners are constrained in the amount of attention they can dedicate to the management of their business. Using two simple interventions, we provide evidence which is consistent with the idea that some business decisions may not be fully attended to. The interventions

[22] A final set of results is in Appendix Table A4. Panel A, as discussed in the introduction, shows that most changeouts end with the customer simply buying the item from another retailer. Panel B shows that inventory management problems are not limited to change - shops commonly stock out of products as well.

make the change management decision more salient and induce the owners to at least partially (cognitively) process the information that was already available to them. The results show that both interventions increased the salience of change management and resulted in fewer lost sales due to insufficient change and a reduction in lost profits.

By focusing on change management, we are able to provide insights into an underlying constraint facing small firms in a developing country context. The particular business decision that we study is surely not the most important factor affecting micro-enterprises' profitability. It is, however, a particularly clean decision to focus on in studying limited attention, since other constraints seem less relevant and improvements are not difficult to ascertain and implement (in contrast to, for example, inventory management). Our results may therefore generalize to other aspects of the business, suggesting that limited attention may be an important barrier to increasing the productivity of small firms in developing countries.

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Table 1: Costs of Holding Inadequate Change

| | Mean | N |
|---|---|---|
| | (1) | (2) |
| **Panel A. Estimates from Weekly Survey** | | |
| 1 if Lost Sale in Previous 7 Days | 0.54 | 507 |
| | (0.50) | |
| Number of Lost Sales in Previous 7 Days | 3.35 | 507 |
| | (6.71) | |
| Value of Lost Sales in Previous 7 Days | 268.71 | 504 |
| | (728.70) | |
| Lost Profit | 68.58 | 496 |
| | (184.90) | |
| Lost Sales when Cash on Hand (But Inadequate Change) in Previous 7 Days | 1.88 | 252 |
| | (2.43) | |
| Lost sale while Fetching Change in Previous 7 Days | 0.45 | 471 |
| | (0.50) | |
| Total Number of Lost Sales while Fetching Change in Previous 7 Days | 2.17 | 467 |
| | (7.09) | |
| Total Time Fetching Change when Could Not Break Bill in Previous 7 Days (Minutes) | 135.16 | 494 |
| | (186.60) | |
| Profits of Business in Previous 2 Hours (Ksh) | 138.82 | 410 |
| | (165.10) | |
| **Panel B. Estimated Lost Profit**[1] | | |
| Estimated total lost profits in previous 7 days | 67.79 | 470 |
| | (183.40) | |
| Total Time Fetching Change when Could Not Break Bill in Previous 7 Days (Minutes) | 134.16 | 470 |
| | (187.80) | |
| Average Estimated Lost Profits[2] | 119.37 | 394 |
| | (205.80) | |
| Estimated Lost Profits as Percentage of Total Profits | | |
| Mean | 0.08 | 394 |
| Standard Deviation | (0.09) | |
| Median | 0.05 | |

Notes: The figures reported are from the first visit with firms. Means are reported in Column 1, with standard deviations in parentheses. There were 508 total firms in the sample but data is missing for some firms on some variables in some weeks (both because the surveys changed slightly over time and because of survey errors).

[1]Figures in Panel B are only reported for firms with non-missing data on weekly profits and estimated lost profits.

[2]This is estimated by lost sales plus time spent fetching change (in hours) multipled by estimated hourly profits (weekly profits / 65 hours per week). Average labor hours per week reported in the background survey was 65.

Table 2: Randomization Check

| | Mean | Coefficient on Information | *p*-value of joint test of wave variables | N |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| Occupation: Fruit and Vegetable Vendor[1] | 0.24 | -0.02 | 0.46 | 506 |
| | (0.43) | (0.04) | | |
| Occupation: Other Retail | 0.37 | 0.03 | 0.69 | 506 |
| | (0.48) | (0.04) | | |
| Occupation: Services | 0.34 | -0.02 | 0.13 | 506 |
| | (0.48) | (0.04) | | |
| Age | 34.87 | 1.11 | 0.49 | 498 |
| | (10.47) | (0.95) | | |
| Female | 0.56 | 0.01 | 0.24 | 494 |
| | | (0.04) | | |
| Married | 0.78 | 0.05 | 0.63 | 496 |
| | | (0.04) | | |
| Number of Children | 3.29 | 0.13 | 0.66 | 487 |
| | (2.50) | (0.23) | | |
| Literate in Kiswahili | 0.94 | -0.03 | 0.51 | 487 |
| | | (0.02) | | |
| Years of Education | 8.85 | -0.40 | 0.28 | 497 |
| | (3.64) | (0.32) | | |
| Years Business has been in Operation | 5.68 | 0.56 | 0.96 | 455 |
| | (5.01) | (0.47) | | |
| Has Bank Account | 0.42 | -0.08* | 0.74 | 498 |
| | | (0.04) | | |
| 1 if Participates in ROSCA | 0.70 | -0.01 | 0.53 | 497 |
| | | (0.04) | | |
| Keeps Financial Records | 0.30 | -0.07* | 0.13 | 492 |
| | | (0.04) | | |
| Employs Salaried Workers | 0.16 | -0.01 | 0.06* | 488 |
| | | (0.03) | | |
| Owns Land | 0.69 | -0.01 | 0.29 | 498 |
| | | (0.04) | | |
| Value of Animals Owned by HH (Ksh) | 21115 | -496.23 | 0.48 | 508 |
| | (33517) | (2987.64) | | |
| Value of Household Assets (Ksh) | 26991 | -1884.90 | 0.17 | 508 |
| | (25597) | (2156.67) | | |
| House has Mud Walls | 0.62 | 0.04 | 0.02** | 498 |
| | | (0.04) | | |
| Ravens Score (percentage correct) | 0.40 | 0.00 | 0.83 | 508 |
| | (0.24) | (0.02) | | |
| Risk: Amount Invested of 100 Ksh[2] | 61.39 | 0.30 | 0.01*** | 497 |
| | (17.61) | (1.57) | | |

Notes: each row (other than the occupation codes) is the regression results of the characteristics in the title column on: indicator variables for treatment wave, a dummy for whether the firm received the information intervention, market fixed effects and business type controls (used in stratification), and a dummy for a refreshed sample in waves 7 through 9. For the occupation variables, the business type fixed effects are omitted. Column 1 shows the coefficient on the indicator for the information intervention, and column 3 shows the *p* value for the joint test of significance of all the wave dummies.

[1]The remaining occupation category not shown in the table is "other."

[2]Any amount invested in the asset paid off 4 times the amount invested 50% of the time and was lost completely the other 50% of the time.

Table 3: Changeouts and Related Lost Sales

| | Had Changeout | | | Number of Changeouts | | | Lost Sales while Fetching Change | | |
|---|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) |
| Veteran Cohort | -0.06*** | | | -0.84*** | | | -0.94*** | | |
| | (0.02) | | | (0.19) | | | (0.22) | | |
| Visit Count | | | -0.02*** | | | -0.16*** | | | -0.08* |
| | | | (0.01) | | | (0.04) | | | (0.05) |
| Sampled for Information | | -0.08*** | | | -0.52*** | | | -0.18*** | |
| | | (0.03) | | | (0.15) | | | (0.07) | |
| Given Information | | | -0.10*** | | | -0.65*** | | | -0.27* |
| | | | (0.03) | | | (0.20) | | | (0.14) |
| Pre-Information Average | | 0.50*** | | | 0.30*** | | | 0.26*** | |
| of Dependent Variable | | (0.04) | | | (0.03) | | | (0.02) | |
| Observations | 866 | 497 | 4828 | 866 | 497 | 4823 | 857 | 495 | 4537 |
| # Firms | 508 | 497 | 508 | 508 | 497 | 508 | 508 | 495 | 507 |
| Control mean | 0.52 | 0.42 | 0.54 | 2.60 | 1.52 | 3.37 | 1.51 | 0.52 | 2.19 |
| Control std. dev. | 0.39 | 0.36 | 0.50 | 4.08 | 2.18 | 6.73 | 4.22 | 1.14 | 7.12 |
| Specification | Mean | Mean | FE-IV | Mean | Mean | FE-IV | Mean | Mean | FE-IV |
| Sample | No Info | Post-Info | Full | No Info | Post-Info | Full | No Info | Post-Info | Full |

Notes: Columns 1, 4 and 7 are regressions of averages of the dependent variable on an indicator for being in a "veteran"cohort (i.e. a cohort that had started in an earlier wave and that had received more visits). The control group are firms that had started being interviewed later, i.e. the novice cohort. Averages are constructed over intervals in which both were being interviewed. Market identifiers and business type controls, variables used in stratification, are also included. Standard errors are clustered at the firm level. Columns 2, 5 and 8 present differences between firms that were sampled for the information intervention and firms that were not, after the information had been given (controlling for averages before the information had been given). Columns 3, 6 and 9 are regressions with firm fixed effects. To deal with partial attrition during the survey period, the number of visits is instrumented with the number of visits a firm should have received by that time. The first stage regression is strong, with a $t$ statistic of over 90. Standard errors are clustered at the firm level. See section 3 for more details.

The mean and standard deviation are for a control group (novice cohorts in Cols. 1, 4 and 7, firms that were not sampled for information in Cols. 2, 5 and 8, and the firms' first visit in Cols. 3, 6 and 9). Standard errors in parentheses.

***, **, and * indicate significance at 1%, 5%, and 10%, respectively.

Table 4. Cash Management

| | Ln Ksh Brought In | | | Ln Ksh Change Brought in | | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Veteran Cohort | 0.13* | | | 0.25 | | |
| | (0.08) | | | (0.17) | | |
| Visit Count | | | 0.01 | | | 0.05 |
| | | | (0.02) | | | (0.07) |
| Sampled for Information | | 0.06 | | | -0.10 | |
| | | (0.10) | | | (0.18) | |
| Given Information | | | 0.11 | | | -0.09 |
| | | | (0.08) | | | (0.20) |
| Pre-Information Average of Dependent Variable | | 0.73*** | | | 0.31*** | |
| | | (0.04) | | | (0.06) | |
| Observations | 865 | 497 | 4805 | 326 | 199 | 1203 |
| # Firms | 508 | 497 | 507 | 199 | 199 | 199 |
| Control mean | 5.25 | 5.41 | 5.19 | 3.14 | 3.55 | 3.13 |
| Control std. dev. | 1.50 | 1.53 | 1.87 | 1.54 | 1.45 | 2.08 |
| Specification | Mean | Mean | FE-IV | Mean | Mean | FE-IV |
| Sample | No Info | Post-Info | Full | No Info | Post-Info | Full |

Notes: see Table 3 for notes on specifications. Exchange rate was rougly 80 Ksh to $1 USD during sample period. There are fewer observations in Columns 1-3 than in other tables because we only asked about the amount of change brought in to work in the latter part of the sample. Standard errors in parentheses.
***, **, and * indicate significance at 1%, 5%, and 10%, respectively.

Table 5. Change Sharing

| | Times Received Change from Other Business | | | Times Giving Change to Other Businesses | | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Veteran Cohort | -2.41*** | | | -1.08*** | | |
| | (0.47) | | | (0.34) | | |
| Visit Count | | | -0.58*** | | | -0.19** |
| | | | (0.10) | | | (0.08) |
| Sampled for Information | | -1.62*** | | | -0.53 | |
| | | (0.51) | | | (0.38) | |
| Given Information | | | -1.73*** | | | -0.87*** |
| | | | (0.44) | | | (0.33) |
| Pre-Information Average of Dependent Variable | | 0.54*** | | | 0.64*** | |
| | | (0.04) | | | (0.04) | |
| Observations | 808 | 495 | 4483 | 815 | 497 | 4449 |
| # Firms | 505 | 495 | 507 | 507 | 497 | 507 |
| Control mean | 13.09 | 9.51 | 14.31 | 9.72 | 7.64 | 10.42 |
| Control std. dev. | 8.94 | 7.55 | 10.59 | 6.31 | 5.84 | 7.47 |
| Specification | Mean | Mean | FE-IV | Mean | Mean | FE-IV |
| Sample | No Info | Post-Info | Full | No Info | Post-Info | Full |

Notes: see Table 3 for notes on specifications. Standard errors in parentheses.
***, **, and * indicate significance at 1%, 5%, and 10%, respectively.

Table 6. Profits

| | Ln Sold Last 2 Hours | | | Ln Profits Last 2 Hours | | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Veteran Cohort | -0.04 | | | -0.06 | | |
| | (0.07) | | | (0.07) | | |
| Visit Count | | | 0.02 | | | 0.01 |
| | | | (0.02) | | | (0.02) |
| Sampled for Information | | 0.03 | | | 0.00 | |
| | | (0.08) | | | (0.07) | |
| Given Information | | | 0.14* | | | 0.12* |
| | | | (0.08) | | | (0.07) |
| Pre-Information Average | | 0.61*** | | | 0.60*** | |
| of Dependent Variable | | (0.03) | | | (0.03) | |
| Observations | 865 | 497 | 4801 | 816 | 497 | 4503 |
| # Firms | 508 | 497 | 508 | 507 | 497 | 507 |
| Control mean | 5.47 | 5.59 | 5.50 | 4.25 | 4.40 | 4.23 |
| Control std. dev. | 1.52 | 1.37 | 1.80 | 1.23 | 1.13 | 1.47 |
| Specification | Mean | Mean | FE-IV | Mean | Mean | FE-IV |
| Sample | No Info | Post-Info | Full | No Info | Post-Info | Full |

Notes: see Table 3 for notes on specifications. Exchange rate was rougly 80 Ksh to $1 USD during sample period. Standard errors in parentheses.
***, **, and * indicate significance at 1%, 5%, and 10%, respectively.

Table 7. Audit

| | Ln Change On Hand | Ln Cash On Hand |
|---|---|---|
| | (1) | (2) |
| Veteran Cohort | 0.39** | 0.66*** |
| | (0.18) | (0.22) |
| Given Information | -0.15 | 0.02 |
| | (0.16) | (0.20) |
| Observations | 522 | 522 |
| # Firms | 199 | 199 |
| Control mean | 3.70 | 5.41 |
| Control std. dev. | 1.97 | 2.45 |

Notes: Data from audits conducted in March-April 2011. See Table 3 for notes on definitions of independent variables. Standard errors in parentheses.
***, **, and * indicate significance at 1%, 5%, and 10%, respectively.

Table 8. Audit Data Quality

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| | Cash on hand in audit | | Change on hand in audit | |
| Reported cash on hand | 1.02*** | 1.01*** | | |
| | (0.02) | (0.03) | | |
| Reported change on hand | | | 0.44* | 0.32 |
| | | | (0.25) | (0.24) |
| Firm Fixed Effects | No | Yes | No | Yes |
| Observations | 522 | 522 | 520 | 520 |
| # Firms | 199 | 199 | 199 | 199 |
| Mean of dependent variable | 1254 | 1254 | 130 | 130 |
| Std. dev. Of dependent variable | 2352 | 2352 | 217 | 217 |

Notes: Data from audits conducted in March-April 2011. Standard errors in parentheses.
***, **, and * indicate significance at 1%, 5%, and 10%, respectively.

Table 9. Buying Change

| | 5% Premium | 2% Premium | 1% Premium |
|---|---|---|---|
| | (1) | (2) | (3) |
| Participated in Project | -0.25** | -0.38*** | -0.17 |
| | (0.12) | (0.13) | (0.16) |
| Constant | 0.40*** | 0.60*** | 0.83*** |
| | (0.10) | (0.12) | (0.13) |
| Observations | 56 | 56 | 39 |
| Overall Mean | 0.21 | 0.32 | 0.72 |

Notes: Data from an intervention conducted in May 2011 in which we offered to sell firms change at the given premium (up to 200 Kenyan shillings). The indicator "participated in project" is equal to 1 for firms that participated in changeout surveys and 0 for randomly selected other firms that did not. Exchange rate was rougly 80 Ksh to $1 USD during sample period. Standard errors in parentheses.
***, **, and * indicate significance at 1%, 5%, and 10%, respectively.

Table 10. Spillovers

| | Lost a Sale | Num Lost Sales | Ln Ksh brought in | Ln Ksh Change brought in | Times Receiving Change | Times Giving Change | Ln Sold last 2 hours | Ln Profits last 2 hours |
|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| Number of firms within | 0.00 | 0.05 | -0.01 | 0.00 | -0.07 | -0.068* | 0.00 | 0.00 |
| 100 meters of business | (0.01) | (0.04) | (0.01) | (0.01) | (0.06) | (0.04) | (0.01) | (0.01) |
| Share Treated Firms within | -0.23 | -5.090** | 0.10 | 0.26 | 7.713** | 4.408* | 0.48 | 0.54 |
| 100 meters of business | (0.16) | (2.31) | (0.59) | (0.68) | (3.70) | (2.24) | (0.67) | (0.55) |
| Mean of Dependent Variable | 0.59 | 3.52 | 5.07 | 3.15 | 16.28 | 10.31 | 5.07 | 4.05 |
| Std. Dev. of Dependent Variable | 0.49 | 7.11 | 1.80 | 2.08 | 11.30 | 7.09 | 2.04 | 1.69 |
| Observations | 197 | 198 | 198 | 197 | 187 | 194 | 198 | 198 |

Notes: Data restricted to the first visit with a given firm. The sample is restricted to firms from the second phase of data collection, in 2011 as we do not have GPS coordinates for the other firms. The regressions also include month dummies. Standard errors in parentheses.
***, **, and * indicate significance at 1%, 5%, and 10%, respectively.

Appendix Table A1. Value of Lost Sales

| | Ln Value Lost Sales | | | Ln Value Lost Profits | | | Lost Sales Cash on Hand | | |
|---|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) |
| Veteran Cohort | -0.32*** | | | -0.25*** | | | -0.22* | | |
| | (0.11) | | | (0.09) | | | (0.11) | | |
| Visit Count | | | -0.11*** | | | -0.09*** | | | -0.02 |
| | | | (0.03) | | | (0.03) | | | (0.04) |
| Sampled for Information | | -0.43*** | | | -0.33*** | | | -0.29* | |
| | | (0.14) | | | (0.11) | | | (0.17) | |
| Given Information | | | -0.54*** | | | -0.43*** | | | -0.44** |
| | | | (0.14) | | | (0.11) | | | (0.21) |
| Pre-Information Average | | 0.53*** | | | 0.51*** | | | 0.41*** | |
| of Dependent Variable | | (0.04) | | | (0.04) | | | (0.04) | |
| Observations | 866 | 497 | 4814 | 863 | 497 | 4792 | 648 | 293 | 1981 |
| # Firms | 508 | 497 | 508 | 508 | 497 | 508 | 436 | 293 | 388 |
| Control mean | 2.69 | 2.09 | 2.89 | 2.02 | 1.56 | 2.14 | 1.61 | 1.56 | 1.89 |
| Control std. dev. | 2.19 | 1.97 | 2.83 | 1.73 | 1.51 | 2.22 | 2.00 | 1.76 | 2.44 |
| Specification | Mean | Mean | FE-IV | Mean | Mean | FE-IV | Mean | Mean | FE-IV |
| Sample | No Info | Post-Info | Full | No Info | Post-Info | Full | No Info | Post-Info | Full |

Notes: see Table 3 for notes on specifications. The exchange rate was rougly 80 Ksh to $1 USD during sample period. Standard errors in parentheses. ***, **, and * indicate significance at 1%, 5%, and 10%, respectively.

Appendix Table A2. Attrition and First stage for Information Intervention

| | (1)<br>At least 1 visit after information given | (2)<br>Received Information |
|---|---|---|
| Sampled for Information | 0.01 | 0.95*** |
| | (0.01) | (0.01) |
| Constant | 0.97*** | 0.00 |
| | (0.01) | (-0.01) |
| Observations | 508 | 508 |
| R-squared | 0.001 | 0.92 |

Notes: the dependent variable in column (1) is a dummy equal to 1 if the firm had at least 1 visit after the date the information intervention was given in that wave. In column (2) it is whether the individual atually received the information intervention.
Standard errors in parentheses. *** indicates significance at 1%.

Appendix Table A3. Cash Management

| | (1)<br>Mean | (2)<br>N |
|---|---|---|
| How do you save money from the business? | | |
| Bank | 0.31 | 237 |
| ROSCA | 0.56 | 237 |
| Home | 0.57 | 237 |
| M-Pesa | 0.22 | 237 |
| Other | 0.03 | 237 |
| | | |
| What do you do with the cash at the end of the day? | | |
| Bring home | 0.76 | 236 |
| Put in bank | 0.05 | 236 |
| Put in ROSCA | 0.31 | 236 |
| Put in mobile money account | 0.13 | 236 |
| Buy items | 0.58 | 236 |
| Restock | 0.31 | 236 |
| Other | 0.05 | 236 |
| | | |
| When do you deposit money into the savings product? | | |
| End of day | 0.41 | 117 |
| Next day | 0.05 | 117 |
| Later in week | 0.46 | 117 |
| Later in month | 0.08 | 117 |
| | | |
| Do you find it hard to save money? | 0.58 | 238 |
| Do you have full control over your money? | 0.90 | 236 |
| Do you mentally separate business and personal money? | 0.64 | 238 |
| Do you physically separate business and personal money? | 0.05 | 237 |
| Do you consume more when your business has a good day? | 0.87 | 237 |
| Do you have another source of income? | 0.29 | 238 |
| Do you have a mobile money account? | 0.77 | 237 |

Notes: data from semi-structured endline interview.

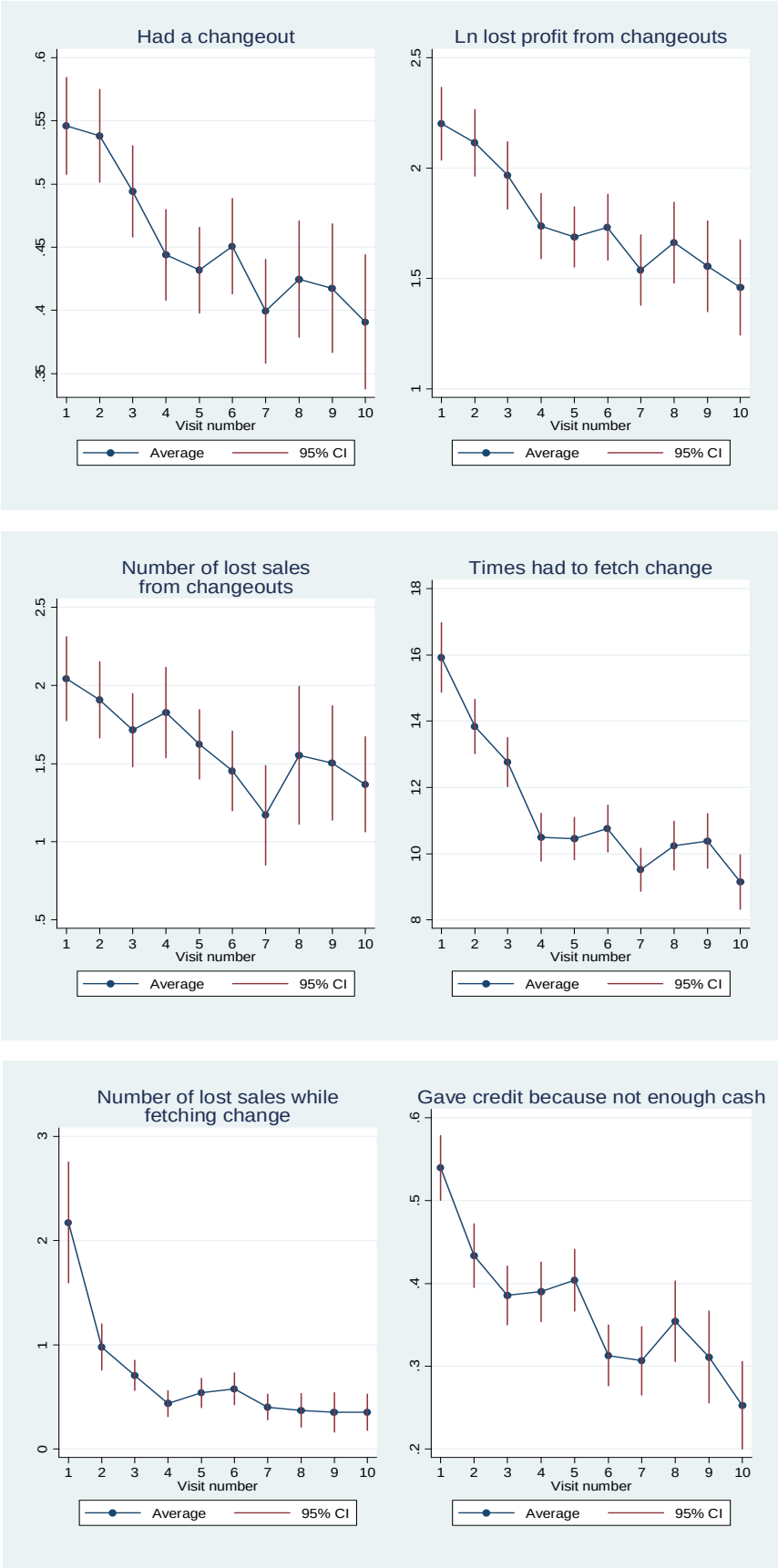
Appendix Table A4. Other information from endline

| | (1)<br>Mean | (2)<br>N |
|---|---|---|
| **Panel A. Costs of Changeouts** | | |
| Do customers ever buy more when you run out of change? | 0.28 | 60 |
| If yes, what percentage of the time | 0.24 | 17 |
| When you run out of change, do you think the customer ever goes home without buying the item? | 0.02 | 60 |
| **Panel B. Other Inventory Decisions** | | |
| Value of inventory (trimming top and bottom 1%)[1] | | |
| Wholesale | 11655<br>(23617) | 455 |
| Retail | 17220<br>(36437) | 456 |
| Did you have a stockout last week? | 0.32 | 237 |
| If yes, total value of lost sales from stockouts | 1241<br>(4623) | 76 |

Notes: data from semi-structured endline interview.

[1]There are many more observations for this variable since the other questions were only asked for a subset of the endline surveys.

**Appendix Figure A1. Trends in Changeouts by Visit Number**



**Appendix Figure A2. Trends in Cash Management and Profits by Visit Number**